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ABSTRACT

Recent research by Coleman, Hoffer, and Kilgore on the effectiveness of public and private schools may be seriously flawed because of its neglect of input-level differences in student performance and its reliance on cross-sectional testing data as the criterion measure. The sample used by Coleman and his colleagues from the High School and Beyond (HSB) data set was limited to seniors and to fewer schools than the more complete data in the National Longitudinal Study (NLS) of the High School Class of 1972. Whereas it is true that a comparison of mean scores for public and Catholic schools in both the NLS and HSB consistently favors Catholic schools, such a comparison may be inappropriate because private sector schools tend to attract students who are in an academic tract, but public schools must take anyone. The differences between public and Catholic test score results become markedly slimmer when academic and general track students are compared separately. When adjusted for differences in enrollment proportions in the two tracks, the figures give only two significant advantages to Catholic schools--in the verbal SAT among academic-track students and in the verbal followup test among general-track students. The differences between public and Catholic schools in achievement scores become insignificant after the variables of student selection and background characteristics are statistically controlled. There is thus little reason to believe that Catholic schools are more effective than public schools in promoting cognitive development. (Author/JW)

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**Report No. 333**
**January 1983**

# PRIVATE SCHOOLS AND PUBLIC POLICY: NEW EVIDENCE ON COGNITIVE ACHIEVEMENT IN PUBLIC AND PRIVATE SCHOOLS

**Karl L. Alexander and Aaron M. Pallas**

The Johns Hopkins University

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This report, prepared by the School Organization Program, analyzes data from the High School and Beyond and the NLS Class of 1972 data sets to examine differences in cognitive and achievement outcome measures of public and Catholic school students within high school tracks. Only small, inconsequential differences are found.

ABSTRACT

Recent research by Coleman, Hoffer and Kilgore on the effectiveness of public and private schools may be seriously flawed because of its neglect of input-level differences in student performance and its reliance on cross-sectional testing data as the criterion measure. Using data from the High School and Beyond and the NLS Class of 1972 data sets, we examine public-Catholic sector differences within high school tracks for a variety of congitive and achievement outcome measures. Even without any controls for sector differences in student characteristics, the public-Catholic differences are all very small. They account for less than one percent of the variance in both test scores and in years of school completed. When student selection and background characteristics are controlled, these small differences shrink even further. We thus cannot agree with Coleman, Hoffer and Kilgore's claim that Catholic schools produce better cognitive outcomes than do public schools. This claim is the first of the "factual premises" that they say would support policies to increase the role of private schools in American education. In our view this premise is wrong, and hence should not be invoked in support of such policies.

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PRIVATE SCHOOLS AND PUBLIC POLICY:

NEW EVIDENCE ON COGNITIVE ACHIEVEMENT IN PUBLIC AND PRIVATE SCHOOLS

Once again, research on schools has evoked great controversy. In a recent study Coleman, Hoffer and Kilgore (1982b) compared the effectiveness of secondary schools in the public and private sectors and found in favor of private schools. This study has attracted a great deal of interest and attention, with much of that on the part of the research community being highly critical. Although Coleman and colleagues have responded to many of the criticisms directed at their work, numerous concerns remain unresolved and recourse to convincing evidence has been exceedingly rare throughout the debate. The present study, too, is critical of Coleman, Hoffer and Kilgore's research, but rather than simply expressing our reservations, we also are able to present original analyses on some of the issues that trouble us most.

Our evidence fails to support one of Coleman, et al.'s central claims--that private schools produce better cognitive outcomes than public schools.[1] Obviously, this is a matter of considerable practical importance. Indeed, Coleman, Hoffer and Kilgore's conclusion that private sector schools are more effective than those in the public sector is probably the central result of their study, at least as judged by the reaction it has evoked. In light of this, it is especially important that the evidence in support of this key point be persuasive. As will be apparent shortly, we find their evidence far from convincing and we find little support for their position in our own analysis.

There is no need here to restate criticisms already made by others. The concerns that most trouble us involve Coleman, Hoffer and Kilgore's reliance upon cross-sectional data and their inability to deal satisfactorily

with the issue of selection biases arising from differences in the mix of students attending schools in the various sectors. Probably the single greatest burden of school effects research is to distinguish convincingly between outcome differences that reflect simply differences in the kinds of students who attend various schools from differences that are attributable to something about the schools themselves. This is no easy task under the best of circumstances, and is especially difficult when students (and parents) select themselves into schools on the basis of criteria relevant to the outcome being evaluated. Put simply, when good students go to good schools, how are we to know which is responsible for the good performance that is likely to be observed?

In the present instance, we know that schools in the public and private sectors differ in their educational missions and academic priorities[2] and in the mix of students they enroll.[3] The standard way to take account of such complications is to obtain measurements on those factors that are thought to be most relevant and to enter them into the analysis as control variables. Hence, differential sector effectiveness would show up as mean sector differences on the performance criterion after adjustment for sector differences in relevant student input characteristics.

Although Coleman et al. adjust for student differences involving socioeconomic background and race/ethnicity, they neglect differences in competency or achievement levels that predate high school. This is a serious omission, one that severely compromises the basic findings of their study. Consider the following points:

1. For large populations the overtime stability in levels of performance on standardized tests of the sort employed by Coleman et al. consistently has been found to be quite substantial.

2. Student background characteristics are only partial proxies for such competency differences.
3. Estimates of school effects on standardized test performance which neglect pre-existing differences in performance levels are likely to be substantially upwardly biased.[4]

In light of these facts, the finding that students in the private sector score modestly better on standardized tests than students in the public sector even after adjusting for student socioeconomic and demographic characteristics is equivocal. If, as seems likely, private sector students are somewhat more capable initially, then Coleman et al.'s results almost certainly reflect at least partly, and perhaps wholly, these initial advantages.

There is another data set well-suited to the issues considered by Coleman et al. which would not have been so restrictive as the HSB survey they employed. This is the National Longitudinal Study of the High School Class of 1972 (NLS). This project includes both private and public schools, it contains information on a great variety of school outcomes, and, perhaps most importantly, it now spans a seven year period. With the NLS data, a more proper evaluation of the public-private question is possible, and it is to the NLS that we now turn for additional evidence on the purported superiority of private schools over public. When possible, we also present HSB results as a base of comparison. The following figures indicate the coverage of both students and schools in the two data sets:

| | NLS | HSB* |
|---|---|---|
| Public: Schools | 1232 | 869 |
| (Students) | (21126) | (24678) |
| Catholic: Schools | 74 | 83 |
| (Students) | (1320) | (2687) |
| Other Private: Schools | 12 | 36 |
| (Students) | (206) | (875) |

*HSB figures are for seniors only. Numbers for schools are obtained from school data files in both instances.

Both studies provide broad national coverage and claim representativeness with regard to their respective cohorts. The greater case base of the HSB, however, offers obvious advantages when focusing on subgroups of special interest and it is better able to withstand the various sources of sample attrition that take their toll in both projects. One very pressing limitation of the NLS is its small number of private sector non-Catholic schools. Because of this, we chose to limit our attention to Catholic and public schools, for which sample coverage is more adequate. Coleman, Hoffer and Kilgore (1982a), however, recently have acknowledged that the sampling of non-Catholic private schools was inadequate in the HSB study as well, and so, as a practical matter, both their study and ours should more properly be presented as public-Catholic comparisons, rather than public-private. For this focus, both data sets are quite serviceable.

Table 1 presents mean scores on the standardized tests from the HSB and on their counterparts in the NLS.[5] Results are reported for Verbal, Math and Reading Subtests and for a composite which is a simple sum of the other three. Results are presented only for seniors in the HSB study. The NLS, as mentioned above, sampled only seniors.

All tests are scaled as standard scores (overall sample mean of 50;

S.D. of 10). Both sets of tests were developed by the ETS and they do contain some identical items (for more detail on the HSB tests, see Heyns and Hilton, 1982). However, apart from our having adopted a common metric, we have made no particular effort to equate the tests across studies. In light of this, the parallels between the HSB and the NLS figures in Table 1 are especially striking.

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Table 1 About Here

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Comparing public and Catholic means, we find a consistent advantage for the latter. Although the differences are not especially large, they all are somewhat greater in the NLS than in the HSB. For instance, in the NLS the mean differences on the subtests are on the order of four-tenths of a standard deviation, while in the HSB they are closer to two-tenths of a standard deviation. In both data sets, the disparity is greatest on the vocabulary test.

These figures, however, may not be the most appropriate comparisons. Schools in the two sectors differ somewhat in their educational goals. Because of this, an argument can be made for focusing on differences between students who are pursuing similar programs of study. Private sector schools tend to be more oriented toward preparation for college; hence, in that sector cognitive skill development should be a more central priority. These differences in scholastic orientation are reflected in the track enrollments in the two sectors. Based on student self-reports, 34 percent of the HSB public school students are enrolled in the academic track compared to 69 percent in the Catholic sector. The corresponding figures for the NLS are 42 and 67, based on school records.[6]

Since academically oriented students are presumably more capable on the average, the overall figures in Table 1 to some extent likely reflect

merely these differences in curricular patterns across the two sectors. We therefore also consider test score results for academic and general track students separately.[7] When comparisons between sectors are made within curricula, the original differences, which themselves were rather modest, become even slimmer. In fact, in the HSB data they become inconsequential. Among non-academic youth, the subtest differences range from just over two points on the Vocabulary test to about one and a half points on Reading. Among academic students, all of the differences are very small, and two actually favor public schools. Neither of these public school advantages is at all large, but the reversal itself is striking. These, it should be recalled, are the same data used by Coleman.[8]

In the NLS data, the general track comparisons are not much different from the overall figures; for academic students, however, the differences are all smaller. Although these latter comparisons still all favor Catholic schools, the differences themselves are trivial. For the subtests, the largest is but two points and the smallest is under one point. The difference between the composite means is only about .15 of a standard deviation.

Table 1 also presents similar breakdowns for the other outcomes which the NLS makes available. These include SAT scores for those youth in the two sectors who took the test,[9] scores on verbal and math tests which were administered to a subsample as part of the 1979 follow-up, and data on years of school completed as of 1979, seven years beyond high school. Before considering the averages themselves, we should point out that they are based on widely varying sample sizes. This is due to differences in their sources. Students with SAT's are self-selected owing to their having elected to take the test, while the 1979 tests were administered to only 11% of the original sample. The number of non-academic Catholic school students

is especially small for these variables. On the other hand, coverage for the years of education measure is much more satisfactory.

These sparse figures are partially offset by the rarity of such data, especially the re-admistration of standardized tests. Additionally, the samples of students taking the SAT in the two sectors, being similarly oriented toward college, may actually be more comparable than even the groups defined by track membership. Hence, this sort of self-selection may compensate partially for the initial dissimilarities between the two sectors. As a practical matter, moreover, the results turn out to be similarly patterned across all outcome measures, suggesting that the varying case base is not a critical consideration.

Despite reservations because of the reduced N's, the NLS figures in the lower portion of Table 1 are highly congruent with those for the cross-sectional test results. The gross public-private comparisons nearly all favor Catholic schools, but in absolute terms the differences are quite small. The one exception involves quantitative SAT scores, where the averages for the two sectors are very nearly identical. More interesting than these gross comparisons, however, are those within nominally equivalent tracks. Adjusting in this way for differences across the sectors in the proportions enrolled in the two tracks has dramatic consequences. Now there are, at most, only two differences of any consequence (the Catholic sector advantage on the verbal SAT among academic students and the Catholic sector advantage on the verbal follow-up test among general track youth), and even these are quite small. The other disparities all are trivial, and four actually favor public school students (three of these involve quantitative tests).

If one accepts the procedures employed in Table 1, then even at this simple descriptive level there is little basis for claiming that private sector schools (i.e., Catholic) produce outcomes superior to public schools. However, for both completeness and conclusiveness, we turn now to a more analytic consideration of these data. These results are presented in Table 2. HSB analyses again are provided as a base of comparison.

For each outcome we report the coefficients obtained for a sector dummy variable predictor across several regression estimations, with all analyses performed separately for academic and general track students.[10] The estimations differ in the control variables they include. The first entry is that obtained with no controls. In standard form, this is the zero order correlation between the sector dummy and the test at issue. This, then, expresses the observed difference between Catholic and public schools. The second entry is obtained from an equation which adjusts for regional and locational (i.e., urbanicity) differences between schools in the two sectors; and, the third takes into account differences in the personal characteristics of students in the two sectors (i.e., SES background, race/ethnicity and gender). Hence, the sector coefficient for the third estimation reflects public-Catholic differences among similar kinds of students, who attend schools in the same area, who also are in the same tracks. Finally, for the long-term outcomes in the NLS, we also present the results obtained when scores on the 1972 test battery are controlled. Since we think the 1972 test results more likely reflect student selection differences than sector effectiveness, this last analysis adjusts for relevant differences in competency levels between students in the two sectors.[11] R-squared statistics for these several equations also are presented. We experimented with a great many variations on this straightforward way of proceeding;[12] the results presented in Table 2 are,

we believe, faithful to the implications of these data in all important respects.

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Table 2 About Here

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The zero-order correlations between our various outcomes and the sector variable all are quite small. These appear in the first column of results for each curriculum. In not a single instance does the sector distinction account for even as much as one percent of the criterion variance--and these figures probably are upper-bound estimates of sector differences! The largest $R^2$ is .008, this being for the vocabulary subtest among NLS general track students.

It appears, then, that even at this gross level sector differences are not large enough to warrant attention. This conclusion certainly applies to the HSB data. For academic track students not a single $R^2$ statistic exceeds .001, and three of the four coefficients are negative (negative signs indicate public school advantages over Catholic). Among general track students, the pattern is a little different because all the comparisons favor Catholic schools and the coefficients are a bit larger than in the academic track comparisons. However, they all still are substantively trivial.

In Coleman, Hoffer and Kilgore's study too the public-private disparities tended to be greater for the general track. The researchers interpreted this as revealing where the private sector's advantage over the public was most pronounced. It strikes us as at least curious, though, that private schools most outrace public schools in the curriculum where cognitive skill development presumably is less salient.

It is possible, on the other hand, that these results merely reflect the superior initial competencies of Catholic school students. Recall that differences in pre-existing testing levels are not taken account of in these comparisons. Although the controls introduced in columns 2 and 3 no doubt adjust partially for such differences, we know from other research that they do not do so fully. It is likely that such uncontrolled variability in initial capabilities will result in upwardly biased estimates of sector differences.

What the data may actually indicate, then, is that the academic track in both sectors attracts students of roughly comparable abilities/achievements, while the Catholic sector tends to enroll somewhat more capable students in its general track. The public schools, of course, must admit virtually anyone who enrolls, whereas Catholic school admissions are selective in at least two respects: first, on the part of the parents and students who choose a non-public option; and, second, on the part of the schools themselves, which retain the right of refusal. For these reasons, it would be quite surprising if Catholic school students were not, in the aggregate, somewhat more competent initially than those in the public schools.

We thus strongly suspect that our analysis, as well as Coleman et al.'s, could mistake selection differences for evidence of differential sector effectiveness.[13] But even if the figures in Table 2 are accepted as evidence of effectiveness, the HSB results in column 1 offer little reason to think Catholic schools superior to public. The effect of adding locational and background controls in the second and third estimations is to reduce the already trivial differences even further.[14]

The NLS results for cross-sectional test scores are highly congruent with those from the HSB: general track differences exceed those for the academic track, but none is especially pronounced; and, the small zero-order differences shrink even further when controls are introduced for locational and student background differences between the two sectors. Also, as was observed in Table 1, the sector disparities generally are somewhat greater in the NLS than the HSB. All these differences, however, involve very small coefficients.[15] The major message of these data thus seems clear: there is little reason to think Catholic schools any more effective than public schools in fostering high levels of cognitive development.[16]

In light of these results from our replication of Coleman's analysis, what we originally thought would be the unique contribution of the present study seems somewhat anticlimactic. Nevertheless, the other NLS outcomes are of interest in their own right, and our findings on them are presented in the lower portion of Table 2.

Differences in SAT performance favor the public schools in three out of four comparisons even before any controls are introduced, but none of these is statistically significant.[17] General track students in Catholic schools score somewhat higher on the verbal test, and this difference is statistically significant (in fact, this is the only significant zero order association in the NLS data). As locational and background controls are entered into the analysis, however, the one Catholic sector advantage shrinks somewhat, while all three public sector advantages tend to enlarge. As has been the case throughout, in no instance are these differences at all substantial.

Three of the four zero-order comparisons for the 1979 test scores favor Catholic schools, but again none of these is statistically significant.[18]

On the other hand, public school academic track students modestly outperform their Catholic school counterparts on the math test. When adjustments are made for locational differences between schools in the two sectors, public students surpass Catholic school students on the math test in both tracks (see the second column of results), and the pattern remains the same when adjustments are made for student background differences.

For these latter outcomes the results for yet a fourth estimation, which controls on students' scores on the 1972 test battery, also are presented. The 1972 test data thus are used as though they reflected student competencies that existed prior to high school. This no doubt overstates the resistance of such traits to academic influence but it probably is more appropriate to use them in this way than it is to use them as school outcomes when one cannot also control for their corresponding "input" values. The latter, of course, is what Coleman, Hoffer and Kilgore have done.

We had expected that adjusting for test score differences in this way would attenuate the sector coefficients obtained when only student background and school location factors were controlled. And, in fact, these test controls do drive down the Catholic school advantage, with three of four differences at this point favoring the public schools. Although there clearly is room for disagreement over whether this is an appropriate use of the Class of 72 testing data,[19] Coleman et al.'s conclusion had been found wanting well before we got to this point. Hence, the general import of our analysis does not hinge on this particular detail of our procedures.

The picture is very much the same for our last dependent variable, years of school completed as of 1979, seven years beyond high school. Among general track students, those attending Catholic schools tend to go

very slightly further through school than those from public schools, but this difference is eliminated when adjustments are made for student background characteristics. Among students in the academic track, attainment levels are virtually identical to begin with. As controls are added, a small difference favoring public schools emerges, but this never reaches significance. Once again, then, we see little indication that private sector schools outperform those in the public sector. This has been consistently the case across many outcome measures and in both data sets.

In light of this striking consistency, there is little reason to think Catholic schools more effective than public schools in promoting high levels of school achievement. There no doubt are many considerations which incline some parents and youth toward private sector schooling, and there might well be good reasons to advocate policies which broaden educational options. At least insofar as the choice of Catholic schools over public is concerned, however, the evidence fails to support at least this rationale for such a preference.

We think it would be a tragic misfortune if opinion and policy regarding the public schools were predicated upon mistaken beliefs. In the present debate over public subsidies for private schooling, Coleman, Hoffer and Kilgore's study is frequently invoked as establishing the superiority of private sector schooling. They themselves, in fact, have framed their work in the context of such policy considerations. Our reanalysis of their data and of other data bearing on the same issue does not support the conclusion that private sector schools are superior. Since this conclusion apparently is wrong, it clearly should not be invoked as a justification for policies to increase the role of private education in American society.

FOOTNOTES

1. Indeed, we believe there is little basis for this conclusion even in their own study. Our reasons for thinking this will be developed shortly.

2. In response to early criticisms, Coleman and colleagues recently have attempted to take account of this by comparing outcomes across sectors within the same curricula, e.g., the academic and general tracks (1982a). This strategy was not employed in either the original technical (1981a) report or the commercially published version (1982b), however.

3. This is the reason for controlling on SES and demographic background characteristics before comparing performance levels between sectors.

4. Evidence on these points is presented in Alexander, McPartland and Cook, 1980, and in Alexander, Pallas and Cook, 1981.

5. Throughout these analyses, the HSB results reflect design weighting. The NLS results are unweighted.

6. School record reports are not available in the HSB.

7. Coleman, Hoffer and Kilgore (1982a) also have performed such within-track analyses. Controlling for track differences in this way presumes that track placements reflect mainly student and family preferences, rather than school policy. Although both factors likely are involved, we believe that curricular placements in most instances are more a function of student and family preferences than of school policy. Indeed, there can be little doubt that many families choose schools on the basis of their curriculum policies. For these reasons, we think it most appropriate that sector effectiveness be evaluated within nominally equivalent tracks. Coleman et al. have conceded that this at least is a proper concern, and have reported results broken down in this way. They also caution, however,

that to the extent that track placements derive exclusively from school policy, this strategy will underestimate sector differences. We readily grant this point, but remain of the opinion that the neglect of track differences altogether would be the more serious failing. We will return to this issue later.

The total figures reported in Table 1 pertain only to general and academic track students. Coleman, Hoffer and Kilgore excluded vocational students because there were so few in the private sector. We have followed their lead in removing vocational students from the analysis.

8. Their study, however, relied upon raw scores and used only the items common to the sophomore and senior year tests. Hence, their results and ours are not directly comparable.

9. Some of these SAT scores were inputed from the ACT subtests. We regressed SAT verbal and mathematics performance on ACT subtests for the nearly 1000 cases in the NLS with data on both sets of tests. These regression equations then were used to predict SAT scores for those students who had ACT scores but did not have SAT data. The prediction equations seem quite adequate, with multiple correlations in the .80 range.

10. We also performed a pooled analysis neglecting curriculum altogether. The results of this analysis were consistent in all important respects with those we present in Table 2. Estimates of sector effects, while still substantively trivial, were somewhat larger in magnitude, however. The largest zero-order association of sector with an outcome was .127 (for the NLS test composite). This might be compared with the largest such zero-order correlation in Table 2, .092 for general track students on the NLS vocabulary test. After regional and student background controls are applied, the largest standardized sector coefficient in the

absence of curriculum controls was .069 (for the NLS vocabulary test). The largest such sector coefficient in Table 2 is .050 (for general track students on the NLS vocabulary test). As we noted earlier, we believe the neglect of curriculum altogether would be a serious omission; hence, we prefer the within-track analyses presented in Table 2. Nevertheless, the results of the pooled analysis differ only slightly.

11. It is arguable whether cross-sectional test scores are more properly used as input controls or as outcomes in research such as this. Coleman, Hoffer and Kilgore have opted for the latter strategy; we believe the case for the former is stronger.

12. We considered these data in a great many ways to assure ourselves that we were not missing something central to the issue of differential sector effectiveness. We performed parallel analyses and regression decompositions (similar to those employed by Coleman, Hoffer and Kilgore) and have tested for various interactions in the regression framework presented in the text. Some of these results offered up minor details that might be obscured in others, but all were consistent on the major question. The analysis we present has the virtue of being uncomplicated and is sufficiently faithful to detail.

13. We should note too that Coleman, et al.'s analysis showed low SES and minority students to benefit most from attending private sector schools. Although this is not revealed in the analysis we present, the same pattern actually appears in both the NLS and the HSB data. However, alternative interpretations based on self-selection considerations could be advanced here as well. This is reminiscent of the "differential sensitivity" hypothesis advanced in Coleman's 1966 study (Coleman, et al., 1966)--that the achievements of minority youth are especially affected by

school quality differences. In neither instance is the evidence in support of such substantive interpretations compelling.

14. We should note that we are more interested in the size of associations and of effect parameters than in levels of statistical significance. In most instances, the sample sizes for these analyses are so large that quite trivial relationships are significant at conventional alpha levels.

15. Although we do not mean to make too much of it, we find it interesting that the sector differences usually are larger in the NLS than in the HSB. If we take this pattern as reflective of selection processes rather than school effectiveness, it might indicate that Catholic schools have become less selective over the intervening decade as they have accommodated the flight from public schools.

16. It actually is hard to tell whether Coleman, et al.'s results are similar in implication, but we tend to think they line up rather well with ours. Since they employ raw-score test results, their descriptive analysis (e.g., mean number right in the various sectors) doesn't provide an anchor by which to judge whether a difference is large or small. Their multivariate analysis employs a regression decomposition strategy, which uses the results from parallel regressions across sectors. Nowhere, then, do we see simple associations between the sector distinctions and the outcomes. However, when we try to look at their data and ours in comparable ways, they correspond quite closely. For example, based on Table 6-2 in Coleman et al. (1982b), the mean differences between the Catholic and public sectors, expressed as fractions of the U.S. total standard deviations, are .41, .26 and .35 for the vocabulary, reading and mathematics tests, respectively. The corresponding number for our standard scores, .40, .26

and .33, respectively, are almost precisely the same.

17. Recall that some of the sample sizes here are quite small.

18. Here too some of the samples are very small.

19. Most research in the status attainment tradition, for example, uses concurrent test data as though they were predetermined relative to other in-school, as well as longer term, outcomes. In fact, a good many such studies employ these same NLS data in this way.

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Table 1

Mean Scores on HSB and NLS Outcomes, By Sector and Within Tracks
(N of cases in parentheses)[a]

| | GRAND MEAN | SECTOR | | GENERAL TRACK | | ACADEMIC TRACK | |
|---|---|---|---|---|---|---|---|
| | | PUBLIC | CATHOLIC | PUBLIC | CATHOLIC | PUBLIC | CATHOLIC |
| HSB | | | | | | | |
| VOCAB | 51.25 | 50.97 | 54.29 | 47.52 | 49.68 | 54.78 | 55.80 |
| READING | 51.19 | 51.01 | 53.08 | 47.74 | 49.27 | 54.65 | 54.33 |
| MATH | 51.44 | 51.21 | 53.89 | 47.10 | 49.18 | 55.71 | 55.42 |
| COMPOSITE | 154.13 | 153.45 | 161.27 | 142.59 | 148.09 | 165.37 | 165.56 |
| NLS | | | | | | | |
| VOCAB | 51.43 (11659) | 51.11 (10808) | 55.53 (851) | 46.12 (4778) | 50.31 (188) | 55.06 (6030) | 57.00 (663) |
| READING | 51.51 (11659) | 51.26 (10808) | 54.69 (851) | 46.32 (4778) | 49.02 (188) | 55.17 (6030) | 56.30 (663) |
| MATH | 51.81 (11659) | 51.54 (10808) | 55.16 (851) | 45.60 (4778) | 48.45 (188) | 56.26 (6030) | 57.06 (663) |
| COMPOSITE | 154.75 (11659) | 153.91 (10808) | 165.38 (851) | 138.04 (4778) | 147.78 (188) | 166.49 (6030) | 170.37 (663) |
| SATVERB | 441.69 (8159) | 440.07 (7382) | 457.08 (777) | 367.43 (1469) | 367.31 (112) | 458.12 (5913) | 472.20 (665) |
| SATMATH | 474.20 (8140) | 474.29 (7364) | 473.38 (776) | 393.84 (1464) | 386.61 (112) | 494.25 (5900) | 488.01 (664) |
| VERB79 | 9.97 (1962) | 9.86 (1834) | 11.52 (128) | 8.06 (890) | 9.30 (23) | 11.56 (944) | 12.00 (105) |
| MATH79 | 14.60 (1962) | 14.49 (1834) | 16.11 (128) | 11.48 (890) | 11.87 (23) | 17.33 (944) | 17.04 (105) |
| EDATT | 14.19 (12766) | 14.16 (11864) | 14.59 (902) | 13.15 (5143) | 13.30 (199) | 14.93 (674) | 14.95 (703) |

[a]Weighted N's for the HSB outcomes, which represent roughly 3,000,000 cases, are not presented.

Table 2

Sector Dummy Variable Regression Coefficients for HSB and NLS Outcomes, Separately by Track and with Various Outcomes

| | GENERAL TRACK | | | | ACADEMIC TRACK | | | |
|---|---|---|---|---|---|---|---|---|
| | PUBPRIV (1) | NEAST NC SOUTH (COMMUN) (2) | SESRAW BLACK HISPANIC SEX (3) | TEST72 | PUBPRIV (1) | NEAST NC SOUTH (COMMUN) (2) | SESRAW BLACK HISPANIC SEX (3) | TEST72 |
| HSB | | | | | | | | |
| VOCAB | 1.867* | 1.375* | .943* | | .861* | .384* | .079* | |
| | (.043) | (.032) | (.022) | | (.030) | (.013) | (.003) | |
| $R^2$ | .002 | .019 | .111 | | .001 | .021 | .142 | |
| READING | 1.148* | .789* | .347* | | -.547* | -.836* | -1.070* | |
| | (.025) | (.017) | (.008) | | (-.021) | (-.031) | (-.040) | |
| $R^2$ | .001 | .010 | .099 | | .000 | .010 | .116 | |
| MATH | 1.893* | 1.153* | .811* | | -.444* | -.834* | -1.022* | |
| | (.043) | (.026) | (.018) | | (-.017) | (-.032) | (-.039) | |
| $R^2$ | .002 | .024 | .137 | | .000 | .018 | .130 | |
| COMPOSITE | 4.908* | 3.317* | 2.100* | | -.130 | -1.286* | [illegible] | |
| | (.043) | (.029) | (.019) | | (-.002) | (-.018) | | |
| $R^2$ | .002 | .023 | .155 | | .000 | .021 | [illegible] | |
| NLS | | | | | | | | |
| VOCAB | 4.211* | 3.479* | 2.306* | | 1.971* | 1.595* | [illegible] | |
| | (.092) | (.076) | (.050) | | (.063) | (.051) | | |
| $R^2$ | .008 | .025 | .150 | | .004 | .022 | [illegible] | |
| READING | 2.678* | 2.245* | 1.034* | | 1.141* | 1.142* | [illegible] | |
| | (.055) | (.046) | (.021) | | (.040) | (.040) | (.028) | |
| $R^2$ | .003 | .010 | .132 | | .002 | .008 | .117 | |
| MATH | 2.862* | 2.255* | .894 | | .826* | .834* | .187 | |
| | (.063) | (.049) | (.020) | | (.030) | (.031) | (.007) | |
| $R^2$ | .004 | .016 | .178 | | .001 | .011 | .176 | |
| COMPOSITE | 9.750* | 7.979* | 4.233* | | 3.937* | 3.571* | 2.201* | |
| | (.083) | (.068) | (.036) | | (.054) | (.049) | (.030) | |
| $R^2$ | .007 | .020 | .205 | | .003 | .015 | .200 | |
| SATVERB | -2.679 | -5.831 | -13.126 | | 14.155* | 12.457* | 9.587* | |
| | (-.000) | (-.017) | (-.038) | | (.043) | (.037) | (.029) | |
| | .000 | .015 | .197 | | .002 | .014 | .134 | |
| SATMATH | -8.887 | -12.254 | -21.121* | | -6.176 | -8.513 | -14.076* | |
| | (-.026) | (-.036) | (-.062) | | (-.018) | (-.025) | (-.041) | |
| $R^2$ | .001 | .017 | .187 | | .000 | .010 | .154 | |
| VERB79 | 1.240 | .947 | .308 | -.241 | .429 | .251 | .150 | .020 |
| | (.054) | (.041) | (.013) | (-.010) | (.043) | (.025) | (.015) | (.002) |
| $R^2$ | .003 | .056 | .192 | .583 | .002 | .024 | .152 | .526 |
| MATH79 | .419 | -.043 | -1.249 | -1.910* | -.305 | -.322 | -.755 | -.953* |
| | (.013) | (-.001) | (-.038) | (-.058) | (-.018) | (-.019) | (-.044) | (-.055) |
| $R^2$ | .000 | .024 | .190 | .461 | .000 | .011 | .194 | .485 |
| ELATT | .195 | .251* | .108 | .023 | .023 | -.032 | -.013 | -.060 |
| | (.027) | (.035) | (.015) | (.003) | (.002) | (-.006) | (-.002) | (-.011) |
| $R^2$ | .001 | .012 | .099 | .179 | .000 | .005 | .103 | .175 |

## Appendix

### Independent Variables Used In Regression Analyses

| Variable | Variable Name | Codes and Sources |
|---|---|---|
| School sector | PUBPRIV | Coded 1 if Catholic; 0 if public.<br>(NLS Var# 004; HSB student item 2) |
| Region | NEAST | Coded 1 if Northeast; 0 otherwise. |
| | NC | Coded 1 if North Central; 0 otherwise. |
| | SOUTH | Coded 1 if South; 0 otherwise.<br>(West is omitted category)<br>(NLS Var# 1066; HSB student item 6) |
| Community size | COMMUN | Coded 1=small; 2=medium; 3=large; 4=very large<br>(NLS Var# 1067; No HSB counterpart) |
| SES | SESRAW | An equally weighted linear composite of standardized measures of father's education, mother's education, father's occupation, family income and household items.<br>(NLS Var# 1071; HSB student item 511) |
| Race/ ethnicity | BLACK | Coded 1 if black; 0 otherwise. |
| | HISPANIC | Coded 1 if Hispanic; 0 otherwise.<br>(NLS Var #1625; HSB student items 416 and 417) |
| Sex | SEX | Coded 1 if female; 0 if male.<br>(NLS Var# 1626; HSB student item 404) |